

Interactive comment on “Intercomparison of aerosol extinction profiles retrieved from MAX-DOAS measurements” by U. Frieß et al.

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Received and published: 11 May 2016

We thank the anonymous reviewer for the constructive comments, which are very helpful for an improvement of our manuscript. In the following, reviewer comments are cited in *italic*.

The authors present an intercomparison of aerosol measurements made during the CINDI campaign held in summer 2009 with emphasis on MAX-DOAS retrievals of aerosol profiles, in particular aerosol extinction profiles and AOT as retrieved from O4 DSCDs. The actual MAX-DOAS retrievals for the different groups are only discussed briefly within this manuscript but sufficient reference material is provided covering the retrieval methods in more detail.

The MAX-DOAS aerosol data sets are then compared with independent aerosol mea-

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surements made during the CINDI campaign as well and the following conclusions were drawn by the authors. First, the MAX-DOAS aerosol profiles are compared to smoothed backscatter profiles from a ceilometer and show good agreement regarding the vertical structure of aerosol in the boundary layer. Second, the MAX-DOAS AOT is compared with the AOT from an Aeronet sun photometer with the MAX-DOAS AOT time series showing overall good agreement with the sun photometer measurements but all MAX-DOAS retrievals systematically underestimate the AOT with potential reasons being briefly discussed in the manuscript. Third, substantial differences exist between the MAX-DOAS surface aerosol extinction when compared to the data measured with the in-situ nephelometer. Potential reasons are discussed but it is acknowledged that the clear disagreement remains largely unresolved.

I have no major comments that need to be addressed. The manuscript is well written, the content is presented clearly, and the paper is recommended for publication in AMT.

Comments to be addressed before publication:

Page 4, line 14: Delete ‘a’ before ‘relatively’

This will be corrected in the revised manuscript.

Page 4, line 15: Replace ‘On the other hand,’ with something like ‘Furthermore’

This will be replaced.

Page 4, line 17: Add one sentence to say if MAX-DOAS can address the issues mentioned above or not, then continue with ‘The usage . . .’

We will add the sentence "Multi-Axis Differential Optical Absorption Spectroscopy (MAX-DOAS) measurements allow for the retrieval of aerosol extinction profiles, and to a certain extent also aerosol microphysical and optical properties, in the planetary boundary layer."

Page 5, lines 1,2: Change to (or rather add) ‘Compared to lidar profiles, MAX-DOAS

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measurements have . . .'

The word "profiles" will be added.

Page 5, line 10: Could add: '... FOV of the receiving telescope of the MAX-DOAS instrument . . .'

The receiving telescope is part of the Lidar instrument, not the MAX-DOAS.

Page 5, line 4 and other places: My preference is to rather use 'group' instead of 'workgroup' as used on page 22, line 25.

The term "workgroup" will be changed to "group" in the revised manuscript

Page 9, line 13: This should read: ' . . .CINDI campaign with their own instrument, but used data . . .'

This will be changed.

Page 9, lines 15-29: Wouldn't it make sense for every group to agree and then use the same or as similar as possible a priori?

The general approach of this study was the intercomparison of extinction profiles from what the individual groups consider as 'best' settings for their retrieval. Therefore no common a priori was defined.

Page 12, line 23: replace 'Our . . .' with *'The MPIC retrieval . . .'*

This will be replaced.

Page 15, line 1: 'golden days' should be explained where first used, currently explained on page16, lines 2-3.

In the revised manuscript, we will state that "golden days" refer to days with predominantly clear sky conditions on the first occurrence of this term.

Page 16, lines 19-22: Please explain briefly if and/or how smoothing the ceilometer

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profiles with the Heidelberg average kernels might have impacted on the comparison study, i.e. how different would the smoothed ceilometer profiles have looked if the averaging kernels of a different group would have been used.

The averaging kernels indeed depend on the choice of the averaging kernel, not only because the a priori is not a true climatology. However, the limited vertical resolution represented by the averaging kernel is mainly determined by the limited information content of the measurements and not by the a priori (see e.g. Frieß et al., 2006). Therefore, we can expect that the resulting convoluted ceilometer profiles using averaging kernels from the different groups are very similar.

Page 18, line 21: Use 'ceilometer' (no capital c, not consistent within the manuscript).

This will be corrected in the revised manuscript.

Page 19, lines 17-22: Sounds somewhat contradictory: the authors write that BIRA, Heidelberg and AIOFM are similar re their choice in a priori but then they point out that the difference between them could also be caused by the different choice of a priori. That needs some rewording or clarification. And it raises again the question if this could be avoided by streamlining the a priori used for the retrievals as much as possible between the groups.

We agree that these statements are contradictory. Furthermore, the a priori of BIRA significantly differs from the a priori of the other groups. We will therefore delete the statement that the a priori profiles are "very similar".

Page 19, lines 20-22: In case of the BIRA retrievals, these should be redone using the some subset of elevation angles the other groups used which - when compared with the original BIRA set (including all the angles in the retrieval) - would then show if this causes some of the differences seen in the profiles or not.

Due to the variety in the retrieval approaches from the different groups, the general approach of the study was the comparison of the retrieval algorithms "as they are", i.e.

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without posing any limitations on the retrieval settings and the input parameters.

Page 20 Figure 7: Somewhere in the discussion should also be mentioned that the AIOFM retrieval gets the elevated cloud layer in the afternoon of 4 July right – actually a very nice example and rather impressive. However, that is not at all the case in the Heidelberg data set which uses the same MAX-DOAS data, correct? Any explanation? Interestingly, the elevated layer is not visible in the ceilometer data set smoothed with the Heidelberg averaging kernels either – any thoughts??? Maybe I missed that but didn't see any discussion in the text.

We will add the following sentence to the revised manuscript: "The clouds apparent in the ceilometer profiles in the afternoon of July 4 between 15:30 - 18:00 UTC are identified in the extinction profiles retrieved by the AIOFM algorithm, but not in the Heidelberg data (no other groups reported profiles for this period)."

Page 21, line 25: typo 'agreement' Page 23, line 23L typo 'do not have'

The typos will be corrected

Interactive comment on Atmos. Meas. Tech. Discuss., doi:10.5194/amt-2015-358, 2016.